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These are my recollections from my earliest days at the Oak Ridge National Laboratory.

I had my job interview after my college graduation in June of 1953. My interview was with Everett P. Blizard, leader of the shielding group in the Physics Division. Art Snell was the director of the division. Harry Maggart was administrative assistant, and he was the first person that I remember meeting. Becky Rickman was Blizard's secretary, so I met her on the day I met Blizard. He invited me to lunch and Herbert Goldstein met us at the cafeteria. He was at Oak Ridge to attend what was called the "Summer Shielding Session." I haven't seen a copy of that report in many years, but I remember that there was a large report from the work of that summer. I returned after my clearance allowed me to come into the laboratory in late September.

I was taken to the Lid Tank Shielding Facility to join the group there. This group was headed by Charles (Chuck) Storrs who was a loan employee from the General Electric Company. At that time all our work was for the Aircraft Nuclear Propulsion program, generally called ANP. The other members of the group at the Lid Tank Shielding Facility were George Chapman, John Miller, and Fritz Norman Watson. The four of us, and were called "engineers," and were the persons responsible for the experiments that were carried out under the supervision of Storrs. The technicians were Jack Money, Jack Taylor, Jim Wampler, and Everett Beckham. We also had electronics technicians Jim Sellers, and Dalton Smiddy. The Lid Tank was located on the west face of the ORNL X-10 graphite reactor. There was a channel through the shield which allowed thermal neutrons from the core of the reactor to come to the source plate of the Lid Tank which generated about 6 watts of fission power. As a source of radiation, it was used to determine the shielding effectiveness of the samples placed in the Lid Tank. These samples were slabs of material, such as lead and steel, which were lowered into the tank of water, and measurements were taken in the water behind the slab samples. Prior to the building of the Lid Tank, the so-called core hole was used as a source of neutrons from the reactor for shielding measurements, which I understand were carried out by Charlie Clifford and perhaps others. There was scattering and leakage around the samples in the core hole so these experiments were not very satisfactory, and hence the tank was installed to alter the geometry of the experiments.

The first of two experiments that I carried out during the year that I was there was a comparison of the secondary gamma-ray effectiveness of lead vs. bismuth. It had been proposed that, due to the

lower thermal neutron cross section, the bismuth might be a better shield than lead, which would result in producing fewer secondary gamma rays. In the experiment, I saw essentially no difference. I gave a paper on this at a classified ANP meeting at Knolls Atomic Power Laboratory (KAPL) the following spring of 1954. The other experiment I performed at the Lid Tank was a measurement in sugar water. As best as I can recall we carried 10,000 pounds of sugar up the stairs at the side of the tank and dumped it into a secondary tank that was placed in the Lid Tank proper. This made a very thick syrup which had to be stirred to keep it uniform. I suspect that it was Herbert Goldstein who suggested that sugar water would provide a medium for carbon to be distributed uniformly throughout a water medium. This experiment allowed us to measure the removal cross section of carbon in a continuous medium as opposed to being in a slab followed by water as most removal cross sections had been determined. The values of these removal cross sections were the main product of the Lid Tank and they were eagerly sought by the shielding engineers of that time. The removal cross section gave an indication of the effectiveness of a particular material embedded in a hydrogenous medium. The theory of removal cross sections required that any material should be followed by material containing hydrogen, such as water. There were other hydrogenous media proposed, such as zirconium or titanium hydrides which were expensive. The Tower Shielding Facility was completed about this time and Jack Money was transferred there. Charlie Clifford and Leo Holland were responsible for the TSF that was going into operation at that time.

In the summer of 1954, Blizard decided to send me to the Oak Ridge School of Reactor Technology (ORSORT) for its 1-year course. At that time there were no nuclear engineering programs available in the universities: therefore, students came from everywhere to get training at ORSORT. There were two categories of students at ORSORT at that time. One category of students was employees of companies who wanted to send individuals for training, the other category was newly graduated students who did not have any employment connections, and they were given a fellowship to attend ORSORT. I was in the first category; Keith Penny and Ernie Silver, were in the second category. Both of these men were offered employment in our division following the 1-year course at ORSORT. The faculty at ORSORT included Bob Meghreblian, who later wrote a textbook from lectures notes at ORSORT, and Jim Marable who gave the reactor analysis course. Blizard gave the shielding course. There were courses on a number of other topics, such as metallurgy and chemical technology. Lewis Nelson gave some lectures in mathematics, and Larry Dresner gave a few lectures. Bob Charpie gave a few lectures, and there were others. In the summer, we broke into groups to study

particular reactor concepts, and various ORNL staff members provided guidance to these groups. Alvin Weinberg was the mentor of our group, which actually worked at Y-12. Like many of Alvin Weinberg's concepts, it was a fluid fuel system.

While I was in reactor school in 1955, the Applied Physics Division was formed. The shielding group was combined with the critical facility group, which I think had been associated with Y-12 prior to that. Thus Dixon Callahan and his group became part of the division and Callahan was an associate division director. Al Simon was also an associate division director. His interests seemed to be mostly what was called Project Sherwood (thermonuclear work). Following ORSORT, a new shield design group formed with Leo Holland as head and Keith Penny and I as members. The prior shield design group had been composed of Pratt & Whitney employees who had been recalled. Leo, Keith, and I took on, as our first assignment, an evaluation of a particular reactor concept to determine the shield weight. As a group, we soon changed our makeup. Fred Keller had come, and he was assigned as leader but was promptly sent to Mexico. I thought when Keller gets back he will organize the group and we'll be told what we should do. Richard (Dick) Maerker was made head of the group for a short time, but he was sent to France. He worked in the shielding group there for about a year. By this time, we were back in the attic of Bldg. 4500 North, and two young Air Force officers from Wright Field: Bill Dunn and Raymond Lessig joined us, and I realized by then that no one was going to direct our efforts. I thought of some research projects to collaborate with Lessig and Dunn.

Clay Zerby and Sid Auslander, (Sid Auslander was a loan employee from Pratt & Whitney), had developed Monte Carlo gamma-ray transport code for the ORACLE, and when Auslander was about to leave, he showed me how to operate this program. So I became the keeper of this program, and I used it for calculations of gamma-ray transport and directed Dunn and Lessig in other studies such as leakage of radiation from the Bulk Shielding Reactor.

After several years, Keith and I moved to the same office. We collaborated on developing gamma-ray transport codes in parallel with Coveyou and his group developing O5R. Quite a few of the ideas for O5R were incorporated into what later became known as the OGRE code. By 1960, Blizard asked me to understudy his lectures at ORSORT and fill in when he couldn't give the lecture. As time went on, I was giving more and more of the lectures until I was giving all of them by the end of the course. Then I took over completely and gave all the lectures for several years (1961 and 1962).

There were 2 projects in the late 1950s that I can remember. The first one began, as I recall very well, when Blizard came to the office to interrupt my work for a few weeks. It turned out to be a number of months. Weinberg and Wigner, at that time were finishing up their book on nuclear chain reactors, and Weinberg was aware of the Lid Tank measurements of the thermal neutron flux distribution which could be transformed to a point source in water. The information could be used to predict the non-leakage probability of an aqueous homogeneous reactor. Weinberg was completing the theoretical description of this reactor in this book. I became involved in computing the function called the buckling which has to do with the shape of the thermal neutron flux in a reactor near its outer edge. Therefore, it would give the leakage or non-leakage probabilities. Reginald Gwin was also involved because of his background in the experiments that had been done at the critical facility where a number of aqueous homogeneous spheres had been taken to criticality. The result of this was summarized in Weinberg's book, a book that Frank Clark always called "V&V" for "Vineberg and Vigner." I remember on one occasion when Weinberg called me into his office and Wigner was there. As this work was discussed, Weinberg left the room leaving me with Wigner. I was awed in the presence of such a famous man. Of course, both Weinberg and Wigner are very gracious and humble people, and I really had nothing to fear. There was a paper authored by Weinberg, Gwin, and Trubey that was given at the Geneva Conference in 1958 by Reg Gwin.

The other project that I remember very well was in collaboration with T. V. Blosser and this concerned the Fermi age of neutrons in water. There was a well-known discrepancy between the theoretical ages based on calculations and the measured age and Herb Goldstein, I think, was behind the pursuit of this discrepancy. The thermal neutron attenuation curve in water from the Lid Tank was essentially unknown close to the source for various reasons. The source had rather large dimensions, and what one wanted was the unperturbed flux from a theoretical point source. The lid tank source, (the original source) was an array of slugs from the graphite reactor. The second source was a better one. It was aluminum with a uranium-aluminum alloy plate as the meat of the sandwich which George Chapman had designed. The power was measured by comparing heat from electrical heaters in the plate to fission heat. The power level was known better than it had been with the original source, but because of the detectors and the size of the fission plate, flux depression was present, and the shape of the curve near the source was really quite unknown. I remember making some attempts to predict what the shape should be near the source based on other information. I remember Goldstein's interest in that, and he pretty well discouraged me that I could succeed in

doing this. So, we took another tack and that was for T. V. Blosser to make measurements in the thermal column of the graphite reactor which was located on top of the reactor. The thermal column consisted of a tunnel through the shield on top of the reactor so that neutrons could come up through the graphite, becoming highly thermalized by the time they arrived at the end of the tunnel. Blosser had various kinds and sizes of fission foils to simulate a point source, and indium foils to measure the thermal neutrons by using cadmium covers which would effectively block the thermal neutrons By subtraction, the data could be interpreted in terms of just the thermal neutron flux. Of course, both the fission foils and detector foils were not point sources or point detectors, and I attempted by mathematical techniques to try to transform the data from a disk source-disk detector to that of a point source-point detector. I worked on some iterative techniques using on the ORACLE. I remember Blizard having some skepticism of my techniques, and he consulted with Coveyou. Coveyou thought it was a reasonable thing I was doing. Of course the ORACLE was a pretty difficult computer compared to what we have now, in terms of programming arithmetic with real numbers. I invented schemes using integer arithmetic which would renormalize whenever getting out of range with real numbers.

Blosser's experiments, resulted in publications mainly in our annual or quarterly progress reports. I don't think these results were ever published in anything other than these, but this problem was being addressed by other researchers, and the work was cited in a number of *Nuclear Science and Engineering* articles. The problem of flux depression arises from perturbation of the flux by the detector. One would like to have a I remember what Frank Clark called a non-perturbing detector. And so, one of the things we did was a series of measurements with foils of different thicknesses to be able to extrapolate to a zero thickness detector. This work was suddenly interrupted when there was an accident. There was an explosion at the pilot plant next door. to the graphite reactor building. This pilot plant building had been used by the Chemical Technology Division to develop pilot chemical separation processes. These pilot plants demonstrated processes that were later used in Idaho for the separation of plutonium. The explosion in one of the cells of the pilot plant deposited contamination all over the area, throughout the graphite reactor building, and around outside. The graphite reactor was shut down at this point, and there was a cleanup operation that took place over a number of months. We never did resume our collaboration on this project. We both got involved in other things. One thing that did come out of this work, was that Goldstein became convinced that the Fermi age discrepancy between measurements and theory was due to the fact that these

correction factors had not been taken into account.

Another thing I remember that happened about that time was when Adlai Stevenson was running for president. I think it must have been 1960. Blosser and I were on top of the graphite reactor looking down, when he was visiting this national historic landmark. I had a great urge to spit on Adlai Stevenson's bald head, as we could see it directly below.

As I recall, it was the summer of 1962 that I first learned about plans for a shielding information center which eventually became RSIC. It was during a break during a division information meeting, and I was talking with Phil Hemmig of the USAEC. Blizard appointed Keith Penny the head of the shielding information center. Keith and I were in the same office, and we began making plans. There seemed to be two immediate tasks: one was the development of a computer-based information retrieval system, and the other was the collection of software. We were aware of computing technology that was being developed, and was apparent that we should be collecting and testing software. We had already done that to some extent. We had obtained the codes NIOBE and RENUPAK from United Nuclear. I made use of RENUPAK, in a number of calculations. RENUPAK was a moments method code that could calculate the neutron distribution spectrum from a point source in an infinite homogeneous medium. I also used a neutron diffusion code called MODRIC. Which had been developed at K-25. RENUPAK, of course, could not calculate the thermal neutron flux so my goal was to see if I could reproduce Lid Tank measurements by using RENUPAK coupled with MODRIC. I used the fast neutron slowing down neutrons from RENUPAK to feed into MODRIC. I was coupling transport theory and diffusion theory, and I called it "Transfusion." I gave a paper on that at an ANS meeting. I remember Hank Honneck, chairman of that session. Another person I met at an ANS meeting through my work with the Lid Tank neutron distributions was Kal Shure. I was flattered and surprised when Kal Shure was on a panel in an ANS session at the Detroit meeting 1956, and he praised the work coming out of the Lid Tank.

Keith and Betty Maskewitz had implemented the NIOBE code on the K-25 computer, and so we were building up a capability to do transport calculations. I recall a request from Japan prompted us to send NIOBE to Japan, and I remember there were many problems that Japanese researchers had with NIOBE. About this time Betty Maskewitz came over from K-25 for an interview, and I

remember Betty coming into the office to discuss prospects of what became RSIC. She was assigned the office that Blizard called Baker Street Irregulars, based on the Sherlock Holmes youngsters who provided him with a variety of information. I remember that great box from General Electric that was parked on Betty's desk containing all the codes from the GE ANP operation which was being terminated.

As we got started at RSIC, Francois Kertesz organized a trip to Battelle. Keith and I went along with Bill Cottrell and Phil Baker who were also starting information centers at ORNL. Battelle Memorial Institute had been involved in information centers for some time, and they had a number of these specialized information centers. The thing I remember best about the Battelle operation was the large rooms filled with filing cabinets which contained cards with extracts, as they called them, from the various documents. Analysts would go through the documents and mark paragraphs which would be lifted for their extracts. These extracts were affixed to the the index cards. Of course, copy machines where very poor in those days, So these copies where not very good. They were replicated as many times as the document was indexed. If you wanted to look up a given category, you would find these abstract cards for the various documents for that category. They would make still other copies to satisfy requesters. I recall requesters complaining about the illegibility of these copies that they might get from Battelle. We asked Battelle if maybe the use of computers would help them, and they thought they would use computers if they could think of a way to use them. Of course, when Keith and I got back to Oak Ridge, we immediately started programming what became SARIS. Peggy Emmett did the programming for what I believe was the first computer-based information system at Oak Ridge. She was already working with us in the programming of OGRE which was parallel to the O5R developments of the Coveyou group.

When Frank Clark came to ORNL, I remember his perceptive observation, that all Monte Carlo neutron transport at Oak Ridge was O5R by definition. Everybody had a collection of subroutines. These subroutines all together were O5R. I remember in the first reports of O5R, it was spelled out with O and 5 R's following, and pronounced ORRRRR, Oak Ridge Random Reactor Research Routine. I think it must have been Loraine Abbott who changed it to O5R.

Incidentally, RSIC was first called the Shielding Information Center (SIC), but we soon thought of the ambiguity there would be in regard to electromagnetic shielding. We put the word "radiation"

in front of it, and it became RSIC.

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